

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

VOL. XLVII. WEDNESDAY, DECEMBER 15, 1852.

No. 20.

THE CONNECTION BETWEEN THE MIND AND THE NERVOUS SYSTEM.

[Communicated for the Boston Medical and Surgical Journal.]

THE prevailing mode of studying the physiology of the nervous system, founded on the idea of the independent vital endowments of nerves, is precisely the same as that pursued in physical science. When the spinal marrow is wounded or diseased, the physiological and pathological phenomena observed, are contemplated in their relations alone and isolated, simply as affections of these vital properties, and not as affections of a compound being, in which a spiritual entity is alive and active in the body, and more especially in the nervous centres. The facts of consciousness and the legitimate consequences which flow from them, are never allowed to interfere with the straightforwardness of reasoning. Now a little reflection will make it plain to any one, that every muscular motion is through an exercise of volition founded on an act of judgment, guided by a sensation. It is not necessary that we should be conscious of every movement or every sensation to establish this fact. Although we are directly conscious of many of them, there are a great many others which have become habitual, and in which the necessary steps pass through our mind so rapidly as not to be recollected, but which make themselves known very readily on a disruption of the conditions on which they depend:—as the difficulty of walking in total darkness; the fact of a person becoming dumb after deafness; the guiding sensations of sound, by which the muscular movements concerned in speech are produced, being removed. Sometimes we are rather unpleasantly reminded of the importance of guiding sensations to direct the judgment in muscular motions, when we seat ourselves in a chair some six inches lower than we estimated it, or endeavor to raise a very light substance which we thought a heavy one. A case is given by Marshall Hall of a man who, having lost the sense of feeling in his feet, was obliged to fix his eye on them in order to stand. The case of a woman mentioned by Sir Charles Bell, who had lost the sense of feeling in one arm, and who could hold her child only when her eye was upon it, proves the great extent, if not the universality, of the principle. Instinctive motions are only habitual motions removed a degree farther from consciousness, and are undoubtedly referable to the same law. To this class belong the reflex motions of Marshall Hall, and in general all those which are

brought about by irritation of the nerves and nervous centres, and are supposed to depend on some vital endowment of nerves. If it is a fact that abnormal irritations of nerves give rise to similar sensations as the physiological stimuli applied to their extremities, it is just as easy to refer the resulting motions to those sensations, as to create new causes, after the example of the advocates of specific properties. Certain classes of motions become associated with certain sensations (at first through the will), in the lower animals, and are attended with such consciousness as they are capable of, are kept up by the law of instinct through the intermediate grades, and finally appear in the higher animals as habitual motions without consciousness.

All that we have a right to infer in regard to the nervous system, is that it furnishes some physical condition necessary in the ordinary state of our being, to enable the mind to be active in sensation and to control the muscles.

If we imagine the simplest form of a nervous circle consisting of a ganglion with a nervous cord dividing and spreading itself, part on the surface of an extremity and part distributed to the muscles which move that extremity, and suppose an impression of touch to be made on it sufficient to excite a movement, what is the character of the phenomena witnessed in that event? Are they of a physical nature? The motion witnessed does not accord with physical laws. It is not in proportion to the amount of force impressed, nor is it in a direction resultant from that in which the impulse came. It is just such a motion as we make when we are conscious of being prompted by desire or aversion, and of judging by the sensation how to direct in order to grasp or repel the object. Undoubtedly the mechanism is the same in both cases. If one belongs to the mind or spiritual principle in the higher animal, the other belongs to that which corresponds to it in the lower. If the latter can be referred to vital properties of nerves, the other can be referred to similar properties of the brain, and the existence of the mind or the "we" is a self-delusion.

But in order for this motion to take place, there must be this nervous circle, and it must exist in its integrity. What, then, can it be for? If it does not respond to the impression and produce the motion, what is it made for? It is a sufficient reply to this, to say that among so many operations as are carried on in the body under the control and direction of the vital principle, it needs this, as a sort of magnetic telegraph to keep itself constantly apprised of whatever disturbs its previous condition. It feels by it the physical impression, and at the same moment the muscle to be moved, and is thus enabled to direct the one by the other. The sympathy established through the function of nutrition, though it may meet the requirements of vegetable life, in which, in a few rare instances, something like motion takes place in remote parts in consequence of external impressions, is inadequate to the wants of animals. An apparatus is developed in them connecting the most distant parts by the highest intensity of apparently inwrought feeling. But this apparatus, while it is thus mysteriously associated with a faculty of the mind, there is no reason to believe, performs any other office than what from

its structure and relations it is physically adapted to perform. It is simply an instrument of feeling.

The simplest form of these associations is between the motions of an extremity and the sensations of touch made on the surface of that extremity, requiring for their production a nervous cord and a ganglion, as in the claw of a lobster when it becomes necessary for two or more such extremities to be united together in order that motions in either may be excited by sensations occasioned by impressions of touch made on the other; the ganglions of all so associated, are united together by nervous cords passing between them. It is thus that the two rows of ganglia which pass up and down the centre of invertebrated animals are evolved and connected, so that in fine it is possible for the mind to move any part of the body as directed by touch. But these motions are very limited and imperfect, as any one may satisfy himself who watches attentively the motions of insects and other invertebrata, so far as they are directed by touch. It is necessary that there should be a great central ganglion or organ, by which the whole nervous system may be controlled; not that any impression should pass to it, nor that any power should emanate from it, to move a muscle. The cerebellum, an organ constructed evidently to enable a large quantity of arterial blood to come into contact with nervous cords, appears by its anatomical relations and physiological experiments, as far as they go, to answer this condition. It keeps up, by the action of the blood as it passes from the arterial to the venous state, a certain degree of tension, probably on all the nerves distributed to the general surface, and to the muscles, which enables the mind to direct its attention to the impression from without, and instantaneously to contract the muscles required.

By thus supposing the cerebellum to be the organ more immediately concerned in enabling the mind to direct muscular movements when guided by the sensations of touch, we have the key to the solution of the mystery which attaches itself to the prevailing theory of the office of the cerebellum, viz. :—That it is the organ for “co-ordinating muscular movements.” The very vagueness and indefiniteness of this expression, which is the only legitimate one that can be drawn from the fact observed, according to the doctrine of specific properties and functions, ought to beget a suspicion that the method pursued is erroneous. All must admit that the body is balanced and kept on its feet by the directing sensations of touch, when sight is withdrawn. And since animals are habitually accustomed to the exercise of the former faculty for this purpose more than the latter, the sudden injury of the organ more immediately concerned in connection with it, would throw them out of their bias at once, and, until they had learned to fix their eyes on the limbs, as in the case of the woman mentioned by Bell, would occasion those irregular and vacillating movements which have given rise to the misty hypothesis mentioned above.

The connection of the cerebrum with the organs of the specific senses is a sufficient indication of its office. Of these senses the sight is so far the most important in its range and influence, that it would hardly be improper to say, that the cerebrum is the organ which enables the

mind to direct the movements of the body, when guided by sight. When we consider the infinite variety and complexity of the movements performed under the direction of sight, by a single small member, the human hand, it is easy to understand this use of the cerebrum, and why it should be larger than that of all other animals, and larger than any other central organ of the nervous system, without supposing it to be endowed with the property of thinking, hoping, fearing, loving or hating. Every mechanic art, all the forms of human labor, penmanship, painting, sculpture, approach perfection only as the muscles of the hand respond to a critical nicety of judgment, founded on sensations reaching the mind through impressions made on the eye.

No less clearly are the functions of the anterior and posterior columns of the spinal marrow pointed out by their anatomical connection. The anterior, which is but a continuation of the cerebrum, connects the latter organ with the muscular system, and is the medium through which the mind by it controls the individual muscles, when it directs movements by the guidance of sight; while the posterior, which passes wholly into the cerebellum, performs the same office in connection with touch. This view will be found to go far to reconcile the discrepancies between the results of the experiments by Sir Charles Bell, and those by Bellingeri, as well as other facts which, according to Carpenter, "have kept alive in the minds of many eminent practical men considerable distrust of the accuracy of Sir C. Bell's conclusions." If the anterior columns and roots are concerned in moving the muscles by sight in their normal state then abnormal irritations of them would give rise to motions in the muscles, and of those motions flexion would predominate. For there is reason to believe that motions of flexion are more associated with sight than are those of extension. On the other hand, if the posterior cords be irritated while sensations predominate, the motions if any would be more likely to be those of extension. It enables us to understand, also, why in tetanus from wounds, and in cases of poisoning from strychnia, where the nerves of touch are the transmittents of the irritation, opisthotonos and spasmodic movements of extension are by far the most frequent.

The larger size of the posterior cord, and the ganglion, indicates that they respectively represent the nerve and ganglion of the invertebrated, and form a complete nervous circle in the higher as well as in the lower animal. The generalizations of Sir Charles Bell were too general, and, like the conclusion that the cerebellum is the "organ for co-ordinating muscular motions," illustrates the insufficiency of the present method of studying the physiology of the nervous system, without connecting it with the mind at every step; or, as it might truly be said, the folly of attempting to act the play of Hamlet, with the character of Hamlet left out.

A singular confirmation of this view is derived from the motions of the iris. This organ receives its nerves wholly from the ophthalmic branch of the fifth pair; a nerve which, in its two superior portions, is the sole representative of the posterior branch of the spinal nerves, completely disconnected from its anterior branch to be found. Now it has been

found by experiment that if this nerve be divided, in some animals the pupil becomes contracted, in others dilated—facts inexplicable, if we suppose the fifth to be a nerve of sensation merely. But if we suppose it to be a complete nervous circle between touch and the motions connected with it, a ready explanation is afforded. It is true some physiologists account for it by the nerve's anastomosing with the sympathetic. But the connection of the sympathetic with the bloodvessels affords less reason to believe that it is a nerve of motion than the fifth. How well this harmonizes, also, with the fact that belladonna applied to the eyelids, conjunctiva, or to the mucous membrane of the stomach, all surfaces in relation with the spinal nerves, produces dilatation; and opium taken internally, and ergot applied to the nasal mucous membrane, give rise to contraction of the pupil.

The existence of five pairs of ganglia at the anterior end of the spinal cord in the lowest order of vertebrated animals, may be thus accounted for. There are three classes of sensations, according to which the body and its parts are principally moved. These are respectively the sensations of smell, sight and touch. For each class there should be a double nervous circle; one to connect the organ with the muscles that move the part of the body in which it is placed (a reflex motion), and another to connect the organ with the muscles that move the whole body; that is, one for motion, and another for locomotion. The ganglions of this double circle for the sense of smell, are the olfactory and the corpora striata. Those for the sense of sight, are the optic tubercles and the thalami. Those for touch, are the corpus dentatum and the ganglions on the posterior cords of the spinal nerves. Thus these latter ganglions stand in the same relation to the corpus dentatum, and through that to the cerebellum, as the optic and olfactory to the thalami and corpora striata, and through them to the cerebrum. The cerebrum and the cerebellum are both but the further development of the ganglionic masses which they cover. The lower forms of the invertebrata are more governed in their motions by the sense of smell, than the higher, and hence the relative importance of the olfactory lobes in them, and their dwindling as we ascend the scale. And if there is any reason why a ganglion should be formed in these lower animals, or, in other words, if provision should be made to bring a mass of arterial blood in contact with the end of the cord in which the nerves distributed to the muscles terminate; there is also reason why further provision should be made in the growth of the cerebrum and cerebellum, for a still larger supply of blood when the limbs are developed, the muscular system more complicated, the motions infinitely varied, and the senses more refined.

We are not driven to the necessity of inferring, as is done by some English physiologists, that the brain is the organ of thought, because there is nothing else for it to do. There is work enough for it to do as the instrument of sensation and muscular motion—or, rather, as the instrument which enables the mind to feel at any moment in the waking state, the external impression whenever made, and the muscles to be moved in accordance with it. And for this reason all the nervous cords

that go to all the muscles, and to all the organs of the senses, are united by white fibrous, *not by gray*, matter, so as to form, as it were, one cord, of which the brain is the end, and the true ganglion.

In my next communication, I shall endeavor to show, from the structure, position, &c., of the brain and nervous system, and from the analogy of other organs, that they perform but one office in all their parts, and that is of a physical nature, and also present some considerations which go to show that the sole office of arterial blood is to stimulate the nerves.

November, 1852.

H.

DEATH FROM THE INHALATION OF A FOREIGN BODY.

BY C. H. HILDRETH, M.D., GLOUCESTER, MS.

[Communicated for the Boston Medical and Surgical Journal.]

For an opportunity of post-mortem examination in the following case, and for so much of its history as came under his observation, I am indebted to the courtesy of my friend, Dr. H. E. Davidson, of this town.

Oct. 25.—The patient—a finely-developed, muscular young man, æt. 17—while passing through the woods, picked off a twig—he was not sure whether of a pine or a hemlock tree—and bit off a portion of it, about an inch in length, which was accidentally drawn into the trachea. A violent attack of cough immediately followed, and the foreign body was felt to “move up and down,” but shortly became fixed. The next day he complained of pain under the right clavicle, “a pricking feeling,” as he described it. Nothing special was discovered upon auscultation, except abundant mucous rales. The patient had had a cough for some days previous to the accident, but since that time had raised some blood, most of the sputa being tinged with it.

28th.—He went “out fishing” on George’s Bank. Cough had somewhat subsided—pain continued. Raised no blood after the second day from the accident, and kept about until Nov. 3, when he was compelled to go below. Pain was at this time severe, but somewhat relieved by firm pressure upon chest—probably by preventing motion of the ribs. Rigors at this time severe, followed by great febrile excitement.

Nov. 4th.—Expectoration, which for the last twenty-four hours had been colored with blood, now suddenly became fetid. About two table spoonfuls were raised at once, attracting the attention of those about him by the exceedingly offensive odor. Pain very severe, by him compared to “the pricking of a thousand pins clear through” his chest. At this time swelling of both lower extremities commenced, and increased to such an extent as to excite fears on the part of his friends “that they would burst open.” This continued until his death, though not to so great an extent, but subsided afterwards.

7th.—The vessel having arrived, patient was put under the care of Dr. Garland, who reports as follows.

“Upon physical examination, percussion revealed great dulness anteriorly on right side of chest, over the fifth rib, and between fourth and fifth—the respiratory murmur was not heard, but broncophony was dis-

ting, and mucous rales were heard quite extensively over right side of chest.

"As the pain was considerable, I ordered the application of leeches, to be followed by poultices over the region affected, and gave calomel, digitalis and ipecac. in powders, to be repeated every three hours; mucilaginous drinks and light diet were also prescribed.

"Monday, Nov. 8.—Called to see patient, about 10 o'clock, A.M. Found him much the same as on the day previous. Had had a restless night, but was a little relieved in respect to respiration. Had perspired considerably, and thirst had been urgent. Expectoration rather copious, but not bloody, though very offensive in character. Pain in side not quite so great; but the symptoms, generally, about the same. I ordered a blister for side, and the continuance of the same medicine as the day before, with the addition of a Dover's powder at night.

"Tuesday, Nov. 9th.—Called to see patient this morning between 10 and 11 o'clock. Found him quietly sleeping in his chair. He soon roused, however, and I learned he had quite a comfortable night. Had had no pain since the blister had discharged. Respiration quite free, cough less troublesome, and expectoration very slight (he expressed himself quite relieved). Pulse 96, less full. Had return of appetite, and but little thirst. The surface being tender from the blister, I learned nothing by percussion, but applying ear to chest, observed no change since last examination, except less of mucous rales. I recommended quiet, the continuance of demulcent drinks, with light diet; the medicine to be administered less frequently, apprehensive of a change for the worse sooner or later.

"Wednesday, Nov. 10th.—I received a message at 7 o'clock, this morning, requesting my presence at the house of patient. On arriving, I found him bolstered up on the bed, laboring for breath, and making expulsive efforts every few seconds to clear his throat from the blood and mucus that threatened every moment to suffocate him. His face and upper extremities were livid; pulse frequent and fast failing. I learned that he rested well till 12 at night, when he took some medicine; then rested tolerably till 3 o'clock, when feeling a little nausea he retched and coughed, and the effort filled his mouth with pus, which continued to be expelled from chest till I arrived. It was evident that nature must succumb. I administered a little carbonate of ammonia in solution, and shortly left the house. He expired in a few minutes after I left."

Autopsy, six hours post-mortem.—Muscular system finely developed. Great breadth and depth of chest. Rigor mortis slight. Much serous fluid flows from mouth and nostrils.

Upon incision through costal cartilages, a quantity of foetid gas escaped from right cavity. Pulmonary and costal pleura thickly coated with recent lymph. Lung adherent to parietes along its posterior surface by remarkably copious depositions of similar character, but easily separable. Interlobular adhesion much firmer. Thoracic cavity contains five or six ounces of a grayish fluid, quite foetid.

At the middle of the superior border of lower lobe is a ragged opening, admitting tip of forefinger, the orifice of a cavity capable of containing

two or three ounces. Upon separating the interlobular adhesion, the cavity is recognized just beneath the pleura of the superior surface of the lower lobe, which, forming its upper wall, exhibits a discoloration, as in idiopathic gangrene, like the stain of nitrate of silver. The external walls of the cavity have collapsed and its fluid contents escaped; the remaining portion consists of detritus of the lung, dark colored and horribly offensive, from among which the largest portion of the foreign body dropped out, having been entirely detached from the surrounding parts. Three smaller pieces were afterwards removed, also loose in the cavity. They were neither pine nor hemlock, but portions of a small shoot of the common red cedar (*Juniperus Virginiana*), the largest an inch and an eighth in length, forked by a small lateral shoot near its terminal extremity.

The walls of the cavity were lined with a well-defined false membrane. There was no induration of the circumjacent pulmonary tissue.

I had supposed there would be no difficulty in discovering the point where the foreign body made its exit from the bronchus, and in tracing its course to the place of its ultimate arrest. But the fact was otherwise. The cavity was situated, as before stated, at the periphery of the lung, and there were no subdivisions of the bronchus extending within several lines of it, of sufficient calibre to admit even the head of a small pin, to which extent they were followed and laid open. Presuming, therefore, that the body must have made its way into the parenchyma of the lung from some large bronchial division (as in Gilroy's case, *Am. Jour. of the Med. Sciences*, Vol. VIII., p. 512), those divisions were re-examined, but no satisfactory traces of its exit could be detected.

The mucous membrane of the larynx, trachea and bronchi presented no traces of inflammatory action—in fact, was rather paler than usual. Right lung, and lower lobe of left, much congested, serous fluid exuding freely upon section. Left upper lobe normal.

The liver, dark colored and surcharged with blood, was of extraordinary size, extending entirely across the abdomen, and as low as the umbilicus.

The pathological condition in this case is evidently analogous to that frequently observed in cases of typhlo-enteritis. As in those cases, the contents of the intestine, evacuated into the peritoneum through an ulceration caused by the presence of a foreign body, produce a fatal peritonitis; so in this case, the contents of a cavity discharging into the pleura, produced a fatal pleuritis.

An interesting point remained uninvestigated—the connection between the pulmonary lesion and the swelling of the limbs. I was not aware of this latter occurrence until some days after the examination, when collecting the history of the case from various sources. It would have been most interesting to have observed if there was phlebitis of the femoral or iliac veins, as is common in the latter stages of phthisis. Acute phlebitis, as is well known, often produces abscesses in the lungs. Is the converse also true? Does acute pulmonary abscess ever produce phlebitis? This case, though not precisely in point, might have afforded

more information upon the subject, and in itself alone have proved highly interesting.

December, 1852.

DR. COALE'S TREATISE ON UTERINE DISPLACEMENTS.

[Concluded from page 373.]

DISPLACEMENTS OF THE GRAVID UTERUS.

To avoid confusion we have hitherto treated only of displacements of the unimpregnated uterus. When the impregnated uterus is displaced, or when the displaced uterus becomes impregnated, some new features are presented which require particular notice.

The chances of impregnation are much lessened by displacement of the organ, though from cases on hand, where the affection existed to a very great degree, and yet it did take place, we must conclude that it is not the simple displacement, but the condition of the uterus which generally accompanies it, that leads to sterility. This condition and this consequence of it seems to have been recognized by Hippocrates in saying, "When the mouth of the uterus is hard, it is also shut"—[54th Aphorism, Sec. v.]. And again, "Women who have the uterus cold and dense, do not conceive"—[62nd Aphorism, same Section].

The treatment of the impregnated prolapsed uterus, when the organ is reducible, is a very simple matter, if indeed any treatment is required. It should be reduced, if it does not readily reduce itself, which, however, is generally the case, and so retained until the fourth month, when it will have increased to such a size as to sustain itself above the brim of the pelvis. This, indeed, ministers to the cure of the disease—provided care be taken after confinement to prevent, by all the other means we have above indicated, a return of the affection. When a protruded uterus becomes impregnated, which has happened now in three or four recorded cases, too familiar even to quote [Portal gives one, *Mem. de l'Academie de Chir.*, tom. iii., p. 369. Chopart, another, *Traité des Malad. de la Vessie*, tom. ii., p. 73. Still later, Perfetti, a third, reported in *Prov. Medical and Surgical Journal*, Dec. 2nd, 1844. The last is very interesting, because after delivery the organ was reduced and apparently a cure effected], the difficulties attending the condition arise from the size and weight of the organ during gestation, and from the hardness and undilatibility of the mouth at delivery. The first is remedied by rest and by mechanical means which will suggest themselves—suspension in a properly contrived bandage. For the last, the knife has been resorted to—enlarging the opening by a crucial incision. This was done in three of the cases recorded, and without great complication of the case, or suffering to the patient.

The anteverted or retroverted uterus is not so liable to become impregnated as the simply prolapsed one—for besides the condition above mentioned of the organ itself opposing it, the neck of the uterus is so placed as to increase the difficulty. When it does, however, become so, it is a much more serious thing than with the prolapsed organ—for there

is generally no tendency in it to rise out of the pelvis, as the period of gestation advances. The consequence is, as the development proceeds, the organs in the neighborhood, particularly the bladder and rectum, become more and more embarrassed, until they are wholly unable to perform their functions, and the result of course is fatal unless effective aid be given. Should, then, a woman, affected with either of these displacements, find herself pregnant, her condition must at once become the care of her physician. Every means must at once be used which may tend to rectify the position of the organ, or at least to raise it out of the pelvis before its increased size makes this impossible, that its subsequent development may cause no such urgent embarrassment in the vital organs, as those just mentioned.

How to effect this elevation of the uterus, does not require particular directions, or indeed any, beyond what are already suggested earlier in this essay. When retroversion exists, there is often difficulty in getting the fundus out of the concavity of the sacrum. We have seen an instrument contrived for doing this, which consisted of a steel conductor to be introduced into the rectum—and upon it, but passing into the vagina, was a rod armed at the end with an ivory pad, intended to act against the fundus of the uterus. This would undoubtedly be serviceable, though we scarcely think a surgeon would need any special contrivance for the purpose.

In anteversion the difficulty of reduction is not so great, and the fundus is more readily reached and passed behind the pubis.

When the development of the ovum has increased to such a degree as to render it impossible to reduce the uterus, and the grave consequences above mentioned are imminent, it becomes of serious moment to know what shall be our resort. Sabatier seemed to think that emptying the bladder by puncture would remove its bulk and thus permit reduction; but we can see that the main difficulty, the size of the uterus, will not thus be affected. Hunter advised plunging a trocar into the organ itself, and reducing its size by giving exit to the amnios. There is no record, however, of such a course being adopted; though judging from the effect of accidental wounds of the uterus and from other parallel cases, we cannot but think it would be unattended with serious consequences, and it surely seems the most rational means suggested.

With a view of saving the infant as well as the mother, Purcell [Capuron—*Traité des Malad. des Femmes*, 1817, p. 287], suggested symphysiotomy, supposing that it would effect such an enlargement of the pelvis that the uterus might be reduced when otherwise it would be impossible. Gardien advocates this strongly, but both are as strongly opposed, and we think with reason.

In one of the cases of anteversion quoted above, as occurring in our own practice, abortion took place regularly at the end of the fourth month—apparently as a resort of nature to get rid of what could be developed no further.

INVERSION OF THE UTERUS.

This is the term given to the condition in which the uterus is turned inside outwards. It may occur suddenly, or by very slow degrees. It

can only take place suddenly immediately after delivery, when the whole organ is a flaccid bag—the body of which may be forced through the mouth, either by pressure behind or by traction through the os, as by the cord. When it takes place by degrees, it is always the effect of the weight of some tumor attached to the fundus, which in its development forces its way through the os, drawing the fundus after it. In this latter case it is evident that the tumor is the main difficulty, and that our contest must be with that—we therefore dispense with any further consideration of inversion from this cause.

Symptoms.—A feeling of sinking and utter prostration comes on immediately after inversion has occurred, even when no flooding accompanies it. This is sometimes accompanied by convulsions, but almost always with those violent, nervous perturbations so frequently attending uterine trouble. These symptoms are generally proportioned in violence to the degree of inversion. With them there is often a most alarming hemorrhage, sometimes destroying life immediately. When several of the above symptoms excite our suspicions of the nature of the mishap, examination must be carefully made for the organ through the walls of the abdomen, and an inability to detect it there will at once of itself convert our suspicions into conviction, especially if, in addition, we find the vagina filled by a fleshy substance. Without the abdominal examination, the latter alone might be taken for another fœtus (breech presentation, for instance)—a polypus, or some other tumor. It may be well to suggest that a polypus has comparatively little sensibility, while the inverted uterus is highly sensitive.

If the organ be not speedily reduced, a contraction and thickening of its walls soon render this impossible. We should therefore lose no time in setting about to effect its reduction. Well oiling the hand, the organ should be grasped and passed back into the vagina. Forming the fingers into a cone, the apex of which is placed against the fundus of the uterus, pressure must be made steadily upwards. This will at first carry the whole organ unaltered further into the pelvis, until the vagina is put upon the stretch. After this it will commence receding, and then with a sudden start pass through the os, and it is again in its usual condition. The hand now must not be withdrawn, but left there until contractions are excited, by which it ought to be permitted to be forced gradually out. This is to prevent a return of the accident.

One point incident to this operation has excited much discussion—the removal of the placenta. Should this be done before the reduction, or afterwards? On the ground of lessening the bulk of the mass to be returned, some of the highest authorities on the subject advise its removal. Others of equally high position—with the plea that the force used in separating the two will irritate the uterus, and that an increase of the hemorrhage may be induced at a moment when every drop of blood is important—recommend that the after-birth should be carried back and the case afterwards treated as one of retained placenta. It seems to us that the course must be determined for the individual case, for we can conceive of the placenta being so bulky as to greatly embarrass if not entirely prevent reduction—and the attachment between it and the uterus

may not be so strong as to require violence and cause irritation in overcoming it. Nauche advises snipping the circular fibres of the os, if they seem to threaten strangulation or to otherwise impede the reduction.

As we above said, the reduction ought to be attempted without any delay, for every moment increases the contractions of the uterus and lessens our ability to turn it back again through the os. The liability of this is so great that even a half dozen hours may put the mishap beyond reach of remedy, though two cases are recorded where reduction was effected after an inversion of several weeks standing [one of these is in the *Am. Journ. of Med. Sc.*, vol. xvi., p. 81].

When permanently unreduced, inversion of the uterus is attended with most painful, harassing and dangerous symptoms. The organ becomes highly sensitive, and is affected with a constant feeling of uneasiness, heightened at times by severe, sharp, lancinating pains. A profuse discharge is soon set up from the inner surface, now turned outward, and is of itself enough to speedily exhaust the vital powers; but in addition to it, frequent hemorrhages still further increase the danger from this source. The os uteri is at all times very much contracted, but occasionally this contraction is greatly increased by inflammation, and strangulation may ensue, causing sphacelus of the whole organ. Even where the circulation is not interrupted sufficiently for this, the vitality of the organ may be so lessened that, as in protruded uteri, ugly sloughing ulcers form in its substance, and thus add another source of suffering, danger and death to the patient.

To ameliorate this horrible state of things, various courses have been adopted by different practitioners, but apparently without any guarantee of success. The only resort which holds out promises of restoring the sufferer to comfort and health, is one which, considering that the subjects of this mishap are generally otherwise in the full enjoyment of their womanhood, seems dreadful, and which could not be justified and would not be tolerated, but for the perfect conviction that it is the *only* resort. This is extirpation of the uterus. The method which has been adopted is by ligature—first drawing the organ down so as to get it as near the labia as possible, and then transfixing the neck just below the os with needles armed with strong silk. The ligatures are tightened very gradually. At first the pain is very severe, and attended by nausea and prostration—which are combatted with opiates and stimulants. In a case otherwise favorably fitted for it, there seems to be no peculiarly serious source of danger in the operation.

The cases recorded exhibit a gratifying return of strength and health after the removal of the cause of exhaustion and disease—and the general state of the system does not seem to be greatly altered—indeed, not even appreciably so in several instances. One patient, Lasserre [*Encyclop. des Sc. Med.* v. xxxvi., p. 179] tells us, “*est restée sensibles aux voluptés conjugales.*”

USE OF THE STETHOSCOPE IN DETERMINING THE PRESENTATION AND POSITION OF THE FÆTUS IN UTERO.

To the Editor of the Boston Medical and Surgical Journal.

DEAR SIR,—In your Journal of Nov. 3d, were copied some suggestions, by Dr. Bell, of Arkansas, on “the use of the stethoscope to determine the positions of the fœtus in utero.” Having had some opportunities of observation in regard to this subject, I offer a few remarks for publication, if you think they would interest any of your readers.

Not long after Laennec had achieved his great discovery, M. Mayor, of Geneva, announced that the pulsations of the fœtal heart could be heard through the abdominal walls of the mother. This interesting fact had excited little general attention, and been almost forgotten, when, four years later, in 1822, M. de Kergardec published the results of his researches upon auscultation in pregnancy, and described the two principal phenomena revealed by this mode of investigation, viz., the sounds of the fœtal heart, and the “bruit de soufflet.” Since that time the subject has excited much attention, and in France, particularly, the results of many important researches have been published. At the present time, in the lying-in hospitals of Paris, the evidence afforded by the stethoscope is regarded as often of the very first importance, and the use of it is constantly taught to the students, as belonging to the first principles of obstetrics.

The most complete and systematic work that has appeared on the subject, is that of M. Depaul, “*Traité d'Auscultation Obstétricale*, Paris, 1847.” M. Depaul was formerly “Interne” at the “Hospital de la Clinique,” where most of his observations were made, and is a pupil of Paul Dubois, an accoucheur of world-wide fame, and personally known to many in our own country, who have enjoyed his valuable “cliniques.”

I propose to give, first, a brief “resumé” of the theory, as laid down by M. Depaul (so far only as relates to the presentations and positions), and then to offer a few observations upon its practical value.

M. Depaul states, first, that there is a point in the uterus, at which the sounds of the fœtal heart have their maximum of intensity, and that by a practised ear, this point may be accurately ascertained. It corresponds in the fœtus to the left scapular region. In the living infant, *ex utero*, the sounds are loudest in the præcordial region. But the position of the fœtus, *in utero*, is such that the back, and not the front, of the chest, is in the closest relation with the abdominal walls of its mother, and the intervening lungs are better conductors of sound before they are dilated in respiration; so that, to the obstetrical auscultator, it is the left scapular region which presents the greatest intensity of sound. This point, he remarks, is nearer the cephalic than the pelvic extremity of the body. Hence in presentation of the head, the sounds will be lower down than in presentations of the feet, or breech, and will gradually diminish from below upwards; while if the breech occupy the pelvis, the sounds will have their maximum higher up, and will diminish from above downwards. If a horizontal line be drawn through the centre of the uterine tumor, in every case of head presentation, the sounds should be at their

maximum below it; and in every case of breech presentation or footling, they should be at their maximum above it.

M. Depaul further states that in presentations of the shoulders, when the dorsal region of the child is in front, we may learn, through the stethoscope, "in which iliac fossa the head is situated, and which shoulder tends to become engaged in the upper strait." The sounds of the heart would be loudest nearer the right iliac fossa when the head is to the right, and nearer the left iliac fossa when the head is to the left; while, in both cases, they would gradually diminish in the horizontal direction.

These sounds are made to determine *position*, too, as well as *presentation*. If a vertical line be dropped upon the centre of the horizontal line, above proposed, the uterus will be divided into four sections—two above and two below. When the sounds of the fetal heart have their maximum in the left lower section, gradually diminishing from below upwards, it is a head presentation, and in the first position (left occipitocotyloid). If the maximum is in the right lower section, with diminution from below upwards, the head still presents, but in the second position (right occipitocotyloid). If the sounds are loudest in either of the upper sections, and diminution from above downwards, it is a breech presentation, and the sounds will be louder on the right or left side, according as the back of the child is turned to one or the other side.

A recent residence in the Dublin Lying-in Hospital (see note at the end) afforded me a favorable opportunity for testing the truth of some of these propositions, and I am convinced that they are of great value. Not that all the propositions of M. Depaul are absolutely and infallibly true—but the evidence which the stethoscope affords is of great importance, and may sometimes decide the diagnosis and influence the treatment. The pulsations of the fetal heart may almost always, if not always, be heard during the later months of pregnancy. The sounds are much louder in some cases than in others, and the extent of surface over which they are heard varies very much. Sometimes they are heard over the whole surface of the abdomen below the umbilicus, and, again, they are restricted to very narrow limits. These circumstances must be greatly influenced by the position of the fœtus, its degree of vigor, the quantity of liquor amnii which intervenes, &c. When the liquor amnii is abundant, the membranes remain entire, and the presenting part is not yet fixed in the upper strait, we readily conceive that the position of the child may be changed more or less, even just before the commencement of labor. Under such circumstances, the evidence afforded by auscultation may be uncertain and unreliable. I have myself found the maximum of sound at the left, when labor had actually commenced, which, a few hours before, was at the right. Before me are the notes of 43 cases observed in the Dublin Hospital, and the only ones of which I have preserved an account. Women are only admitted into the institution when labor is believed to be commencing, or about to commence, and the observations were made indiscriminately among them. In some cases the labor had commenced, and the liquor amnii had been discharged, while in others this had not taken place.

Let us suppose the mother's abdomen to be traversed by two imaginary lines, crossing each other at right angles—one vertical, piercing the umbilicus, the other horizontal, passing just below it—and we have divided the whole surface into four spaces, two lower and two upper. Of the 43 cases just mentioned, the maximum of sound was found in—

1. The left lower space in	- - - -	28 cases.
2. The right lower space in	- - - -	5 "
3. The median line below-umbilicus in	- - - -	3 "
4. The left upper space in	- - - -	2 "
5. The right upper space in	- - - -	2 "
6. The median line above umbilicus in	- - - -	1 "
7. Having two distinct foci in	- - - -	2 "

The first 28 cases, examples of the first position of the head, left occipito-cotyloid, go plainly to confirm the rule. So do 3 of the 5 cases in the second class, which were instances of the second position of the head, right occipito-cotyloid; while of the other two, one was a premature birth at seven months. In the 3 cases of the third class, the sounds having their maximum at the median line, and diminishing equally on both sides of it, the presentation was indicated, while the position was not. Of class four, in 1 case the presentation was a breech, according to rule; in the other, a head, contrary to rule. In both cases of class five, the presentation was of the head, contrary to stethoscopic indications. In one of these cases the liquor amnii was noted as unusually abundant. The case in class six was also a head presentation. In this instance, I remember that the observation was made a day or two before the commencement of labor. Both the cases in class seven proved to be of twins, as indicated by auscultation. In the first case of twins, one maximum was to the right, above the level of the umbilicus, and the other in the left iliac fossa; in the second case, one was in the centre of the lower right space, and the other far over on the left side. In the first instance, again, the sounds could be perceived all along a line extending from one focus to the other, though very faintly in the centre of it, and increasing towards each extremity. In the second case, it was quite lost in the interval.

Out of the forty-three cases, then, there are six which seem to contradict the rules which should apply to them, while a very large majority go clearly to sustain them. In connection with several of these six exceptions, some peculiarity of circumstance has been noted. Further, it seems to be extremely probable that if auscultation had been practised, in every case, after the commencement of labor, and when the presenting part had become engaged in the superior strait, the exceptions would have been, at least, less numerous, and, perhaps, not more than are found to most general rules. It will be remarked that the observations here given only include cases of presentation of the head, in different positions, presentation of the breech, and two cases of twins. The impressions remaining in my mind of other observations, of which no notes were preserved, harmonize very well with the results of these.

M. Cazeau writes that his own experience permits him to regard it as

certain, that "when the pulsations of the heart are perceived low down, in front, and to the left, the head presents, and in the first position, left occipito-cotyloid: that when they are heard low down, in front, and to the right, the head still presents, but in the second position, right occipito-cotyloid; and, further, a breech presentation may be suspected when the pulsations are loudest at the level of, or above the umbilicus." He does not think that presentations of the shoulders can be precisely diagnosed by auscultation alone, as proposed by M. Depaul.

It seems to me probable that much of the uncertainty which has attended observations of this kind, is owing to the fact that they have often been *made at too early a date* to determine the final position of the fœtus. Until the presenting part is actually engaged in the upper strait, the position of the child may change more or less from day to day. But let the observations be uniformly made when the labor has actually commenced, and *the presenting part has taken its final position at the superior strait*, and I am convinced the results may be regarded as of much and definite value.

I have confined these remarks to auscultation as a means of diagnosing the presentation and position of the fœtus in utero, only detailing, besides, two cases of twins. Its value, as affording a certain sign of pregnancy, and in indicating to the accoucheur the appropriate time and manner of interference in many cases of unnatural labor, cannot here be spoken of.

Most respectfully yours, I. T. DANA, M.D.

Portland, Nov. 16, 1852.

N. B. The Rotunda Hospital, Dublin, is the largest, of its kind, in the world, I believe, excepting only that at Vienna. Since its opening, in 1757, not far from 170,000 women have been confined in it, and more children have first seen the light within its walls than would constitute the entire present population of Boston.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, DECEMBER 15, 1852.

Dr. Hooker's Inaugural Address.—After achieving a reputation as an author of which any professional gentleman might be proud, Worthington Hooker, M.D., formerly of Norwich, Ct., was elected to the chair of Theory and Practice in Yale College—and a wise election it was, on the part of the trustees. It is so rare, in these days of political expediency, to see a person elevated to a place of honor, solely on account of his eminent qualifications, that the circumstance is regarded almost as a phenomenon, and therefore worthy of a special record. Dr. Hooker was taken on the score of excellent qualifications. He deports himself at New Haven admirably. He can build up and sustain the school, if any one can; and the friends of the institution have reason to congratulate themselves on the appointment. But we have before us the doctor's inaugural discourse, an agreeable production, smooth as a summer's sea, all the gems at the bottom being distinctly seen as the reader glides over the surface of its

many fair pages. It is desirable that the production should be extensively read, and we therefore refrain from extracting sentiments or paragraphs, that should be studied in connection with the whole. Dr. H. manifests a professional independence that is truly praiseworthy, and his whole address interests while it instructs. Fearlessly as the author attacks the absurdities and crudities of the medical ranks, he is never ferocious, or in the least degree unjust. Were such traits discoverable, we should as quickly complain, as any other aggrieved member of the medical fraternity. Dr. Hooker is for unabated effort in progress and reform. "Then will the prospects of our science be bright indeed, and our profession will labor without embarrassment, and therefore with abundant success, in its appropriate work of observation."

Dr Preston's Address.—There are several connecting historical links introduced into this discourse, which mark specific periods in the history of medicine, of a curious as well as instructive character. If we differ from the author in regard to theoretical doctrines, there is no earthly reason why he should not be commended for his industry and tact in arranging chronological memoranda, illustrative of the ever-changing views of men on the subject of medicine. Mrs. Willard is respectfully referred to as having "supported her theory by many ingenious experiments." "If," says Dr. Preston, "the new-born infant does not breathe, the blood does not circulate—the heart is still as death; if it does breathe, the heart moves, the blood mounts upward with the pulses of life—and man becomes a living soul." There is a thought suggested in the 43d page worth looking at very deliberately. "Our laws proscribe no one who has the means to purchase a title—why should our institutions?"

"Why should they take the pains?
Purses are heavier, sure, than brains."

Letters on Syphilis.—Every study has its appropriate literature, in these times of subdivision of labor. A concentration of thought and action upon one subject brings out all there is to be known about it. We are beginning to appreciate the advantages accruing from following specialties in practice, and also of conducting inquiries into any art or mystery, whether it relates to the nature of disease, or the organization of an animal. But there is nothing new in all this, since the ancient Egyptians, according to the Father of History, originated the idea that perfection resulted from concentrating the powers of an individual upon one point, instead of a number;—"hence there was a physician for the teeth, another for the ear, and a third for the eye," &c. Mr. A. Hart, of Philadelphia, has published a neat octavo, embracing an English translation of M. Ricord's letters on syphilis, with an introduction by Amédée Latour, and translated from the French by W. P. Lattimore, M.D. The work is admirably printed, with a clear, good type, and we doubt not its favorable reception by the profession. Comments on Ricord's writings would be quite absurd. He has the entire ground in his peculiar study, and stands out before the medical world, in bold relief, as the most eminent authority on the malady of syphilis.

No necessity exists for referring to the translation now being published in this Journal, by Dr. Slade, of the same series of letters, as the reader has them before his eyes; and those of them who procure Dr. Lattimore's

translation can readily compare the two. Either are good enough—for the purpose contemplated is simply to relate, in English, M. Ricord's doctrines, as delivered by him in France.

Mass. College of Pharmacy.—Dr. Charles T. Jackson has commenced a course of twelve lectures in Boston before this institution. Every person who is permitted to dispense drugs and medicines, should have the opportunity of hearing this learned chemist. It will very much redound to the reputation of the college, to have an annual systematic series of lectures by competent men. This is an age of intense scientific activity, and it would be a reproach to us in Boston, were the apothecaries among us negligent in respect to the professional education of those who are learning the art and mystery of the drug-business.

Physicians for Australian Ships.—Young professional gentlemen who would like to make the round of the world, by taking a voyage to Australia, are likely to have opportunities. There is no compensation beyond payment of all personal expenses. We were applied to last week to point out a physician for a vessel nearly ready to sail. It would be entirely a trip of pleasure.

Opium Trade.—In 1850, Dr. Nathan Allen, of Lowell, Mass., wrote a pamphlet on this subject, embracing a sketch of its history and effects, as carried on in India. Having found its way to that country, it appears to have been the basis of a profound article, in the Bombay Telegraph, which has recently appeared in the Living Age. Dr. Allen seems to have embraced the whole ground, even to the moral evil growing out of the reckless system of trade in the hands of English capitalists. We are gratified with the discovery that an American physician is actually better authority than any of their own people, in respect to the statistics of drugging China with the seeds of death. Forty-five thousand chests are smoked in China, annually. At seventeen grains, daily, to a man, a fair average, there must be four millions of opium smokers to consume this quantity!

Irregular Regulars.—Circulars, printed notes, and other happily conceived devices for arresting the attention of people who have no time for reflection, are flooding this part of the country, recommendatory of the pills and powders of certain individuals, who are represented as gentlemen of established reputation, and therefore above suspicion. There is a prodigious amount of quackery conducted under the guise of a *strong sense of duty*, which compels certain physicians to disengage themselves from the trammels of society discipline, and strike out an independent course of their own, simply to put into the hands of the great public some remarkable preparation that is quite unknown to the ordinary medical brotherhood. Many a fortune has been amassed by a come-outer; but it is the meanest of all courses to turn traitor and then set up the apology of a prompting conscience. We thought of individualizing a few of the most conspicuous of these conscientious speculators in health, whose heralds, in the form of pamphlets, appeals, notifications and cautions, in the course of a week have collected on our table. But cataloguing them would rather promote the objects of their proprietors, and the idea, therefore, was abandoned. Under

this peculiar aspect of affairs medical, what can be done in the case of those who put all rules of professional propriety at absolute defiance? Presuming that others have contemplated the erratic course of many a recusant brother, what course, in their opinion, can be devised to uphold the respectability of the medical profession, and preserve it from the contempt of well-directed minds? The question is open for discussion.

The Mind and the Nervous System — The Nerves of the Iris. — A note from the author of the first article in to-day's Journal, requesting a correction of some remarks on page 418, was not received till after that part of the Journal was put to press. The insertion of the following note in this place is all, therefore, that we can do in the way of correction the present week:—"The ciliary ganglion, which supplies the iris, receives filaments from three sources,—the third, the fifth, and the sympathetic. Of these the third in reality corresponds to the anterior cord, inasmuch as the mind regulates by it the motions of the iris according to impressions of light made on the retina; the fifth performs the same office for tactile sensations, while the sympathetic is used for controlling the blood-vessels."

Medical Miscellany.—Quite a revolution is going on in the professorial chairs in Philadelphia, according to Dr. Bryan's Journal. Medical students have poured into that city in surprising numbers the present lecture season.—Dr. Cotting's discourse on Nature and Disease, has been issued in a handsome pamphlet form.—There are 145 students at the medical course in the University of Michigan, and the number is constantly increasing. Tuition free.—When the lectures commenced at the Medical College of Georgia, 200 students were present.—Dr. John Bell has reestablished himself in Philadelphia, where he will soon issue a work on mineral waters.—The yellow fever is still raging most destructively at St. Domingo.—Small-pox is prevalent in certain towns in Worcester co., Mass., says a correspondent.

TO CORRESPONDENTS.—A communication from New York on the subject of the new operation on exposed dental nerves has been received, but not examined. If relating to the controversy respecting priority of discovery, it will be inadmissible. A short paper on this latter point, alluded to last week, designed to reconcile the conflicting claims of the two parties, has been crowded out of to-day's Journal, but will be inserted next week, and it is hoped will finish the controversial part of this subject.

ERRATUM.—The Steamer Cana-la, with Dr. Channing on board, was not escorted out of Liverpool in September by so large a fleet of steamers as was represented in last week's Journal, at page 400. The number 107 should have referred to the Canada's "company," or officers and crew.

MARRIED.—At Springfield, Vt., Alvah R. Cummings, M.D., to Miss Mary C. Davis, both of Acworth, N. H.—At Abington, Ms., Dr. A. P. Chase, to Miss Deborah C., second daughter of the Rev. A. P. Howland, all of A.

Deaths in Boston—for the week ending Saturday noon, Dec. 11th, 71.—Males, 36—females, 35. Abscess, 1—accidental, 2—inflammation of brain, 2—consumption, 11—convulsions, 1—croup, 3—dropsy, 1—dropsy in head, 1—infantile diseases, 6—exhaustion, 1—erysipelas, 1—typhoid fever, 4—scarlet fever, 15—hooping cough, 3—disease of heart, 2—inflammation of the lungs, 7—old age, 4—purpura, 1—disease of the spine, 1—teething, 1—thrush, 2—unknown, 1.

Under 5 years, 35—between 5 and 20 years, 12—between 20 and 40 years, 13—between 40 and 60 years, 2—over 60 years, 9. Americans, 35; foreigners and children of foreigners, 36. The above includes 3 deaths at the City Institutions.

Hullihen's Operations for Exposed Dental Nerves. TO THE EDITOR, &c. Dear Sir.—The unfairness of the whole of Dr. S. P. Miller's article relative to Hullihen's Operation for Treating Exposed Dental Nerves, in your Journal of Nov. 17th, compels me to seek an opportunity through the same channel to reply, so far as the article relates to priority of discovery.

I have nowhere furnished Dr. Miller with the inference that Dr. Hullihen had no evidence of the treatment of exposed dental nerves by his operation previous to the winter of 1850-51; but on the contrary, the whole tenor of my paper published in your Journal of Nov. 10th, contradicted any suspicion that might arise, that Dr. Hullihen concealed this operation, until he had filed a "written description" with his "legal adviser," or for any sinister purpose. The attainments and eminence of Dr. Hullihen, as a dentist and surgeon, would also not only forbid the probability of his harboring professional secrets, but would lead to the conclusion that this operation, to which he attaches so much importance, would, early in its discovery, be made the subject of scientific inquiry and debate, with such gentlemen of medical attainments, as he most frequently met. In confirmation of this, I append the following letters from professional gentlemen which most conclusively establish the claims of Dr. S. P. Hullihen, which have been assailed by Dr. Miller.

Wheating, Nov. 20th, 1852.

DR. S. P. HULLIHEN. Dear Sir,—In reply to your request, that I should state what I know in relation to your performance of an operation to relieve or prevent the pain of exposed dental nerves, which operation is described in Dr. Cone's report to the American Society of Dental Surgeons, and called by him "Hullihen's Operation," I will briefly say, that you have performed the operation three times upon my own teeth:—first, in the summer or fall of 1846; second, in the fall of 1850; and third, in the summer of 1852.

I may further state, that for the last six years and a half, our offices have been in the same building, and before that, for several years they were very near together,—and that from before the time you performed the first operation on me, I have through your invitations, had frequent opportunities to witness the performance of the operation, as well as to examine cases in which the operation had previously been performed. I have also on several occasions, conversed with you in regard to the probable explanation of the phenomena produced by the operation, which operation, singular as it may appear, prevents pain in a tooth, although the plug may press firmly upon the exposed and tender nerve.

Very respectfully,

JOHN FRISSELL, M.D.

Allegheny city, Pa., Nov. 18th, 1852.

DR. S. P. HULLIHEN. Dear Sir.—In answer to your letter of the 14th inst. inquiring what knowledge, if any, I possess in relation to the operation of drilling into the nerve-cavity of a tooth, called in Dr. Cone's late report, "Hullihen's Operation," I beg leave to say that I entered your office as a student in the spring of 1846, where I remained for three years; that even during the first part of my pupillage, I witnessed the operation in question by you, several times, and since my location in this city in 1849, I have been performing the operation with entire success.

Very respectfully yours,

DANIEL BOISOL, Surgeon Dentist.

Pittsburgh, Nov. 18th, 1852.

DR. S. P. HULLIHEN. Dear Sir.—Your letter soliciting a statement of what I know respecting an operation upon exposed dental nerves, particularly described in Dr. Cone's last report to the American Society of Dental Surgeons, under the name of "Hullihen's Operation," reached me this morning. In reply I have only to state, that I was a student in your office from the autumn of 1846 up to 1850; that I had frequent opportunity of seeing you perform the operation referred to; that you taught your students to esteem it as one of the most valuable operations in dental surgery; that you performed the operation upon one of my teeth, and gave me the opportunity, while in your office, to perform it upon others.

Yours respectfully,

W. F. FUNDENBERG, M.D., Dentist.

Other equally satisfactory proof could be forwarded to you, and if the testimony of unprofessional individuals was to be taken, a score of similar letters could be offered to sustain the claims of Dr. Hullihen.

I must decline being held responsible for Dr. Miller's obtuse mental faculties; but will assure him, that the gross piracy and plagiarism, which, it would appear, his own guilty consciousness has forced him to publicly deny, before publicly charged on him, shall be most fully discussed in a dental journal, agreeable to his not very courteously expressed wish.

C. O. CONK.

Baltimore, Nov. 25th, 1852.

Air-Bed for Invalids.—At a late meeting of the Medical Society of London, a new air-bed, made at the suggestion of Dr. Hawksley, and said to combine several very useful desiderata for the sick and bed-ridden, was exhibited by Mr. Bax, of Charing-cross. We have an opportunity now of mentioning only its most important and original feature, that of affording the means of carrying off effused fluid, whether the product of fomentations, baths, or urine from incontinence. This is managed by means of a vulcanized india-rubber tube, with a somewhat funnel-like commencement, being made to penetrate the bed through its centre, and which, opening when in use, being the lowest part of the surface, effectually drains off all fluid. It is likely to be very useful for old and enfeebled subjects.—*London Lancet.*

"Grooms are the most practical physiologists concerning digestion. If their master's horse comes in over-jaded by a long run, they give him a little water and half an hour's rest before they put in the carbon and nitrogen (oats and beans), well knowing that the horse would not feed, or feeding would become dull, because his 'powers of stomach' 'aren't come back again yet.'"